OBSERVATIONS
ON THE
Progress and Tendency
OF SOME OF THE
MODERN METHODS
OF
SCIENTIFIC RESEARCH.

BY

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Gentlemen,—It has sometimes been the custom for the President at our annual gatherings to review some portion of the past history, or some phase of the present condition, of homœopathy in its relation to the profession of medicine as a whole. This has been so well done by those that have preceded me, and the ground in this direction has been so completely occupied, that I have ventured to-day to pursue a somewhat different course.

It has been the custom with me, now and then, to review my position in regard to my faith in the principle of *similia*, and more especially in regard to my belief in the action of minute quantities of any agent that was supposed to produce a given effect. In doing this, whilst not neglecting what is strictly within the domain of medicine, I have sometimes gone beyond this in order to see what conclusions can be drawn from the experiments of workers in other departments of scientific research in support of the belief in the action of the small doses of drugs the adherents of the homœopathic system of medicine use in the cure of disease. It is to these *observations*

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* Being the Presidential Address delivered at the Congress of Homœopathic Practitioners, held at Bournemouth, September 18th-19th, 1890

on the progress and tendency of some of the modern methods of scientific research that I shall ask your attention for a short time to-day.

To most, if not all, of those present, the facts I have to bring under notice will be more or less familiar, so that I cannot hope to bring much that is new before you; but if I can group them in such a way that they will produce on each of your minds the same effect they have had on my own, they will help to strengthen your belief in the action of small doses, whilst they will, at the same time, help to show that this belief has a solid foundation of scientific facts to rest upon. In passing these under review I shall not attempt to place them in chronological order, but rather permit them to group themselves in a way that will best answer the object I have in view.

The first subject I shall bring under your notice is that of *Spectrum analysis.* In no department of scientific investigation has there been, in the earliest experiments, less indication of the vastly important results that later investigations would reveal than in this. Some of the earliest experiments were made by Sir Isaac Newton more than two hundred years ago. By passing a small beam of sunlight through a circular aperture into a darkened room, and thence through a glass prism, Newton found that the apparently homogeneous white light was resolved into a band, or spectrum, of various colours. He also found that these coloured rays always occupied, in each case, a given position on the spectrum.

More than a hundred years now elapsed before any important advance was made in this subject. In 1802, however, Dr. Wm. Wollaston determined to try the experiment in another way. Instead of passing the light through a circular aperture, he passed it through a narrow slit. Wollaston found, as Newton had done before, that the beam of white light was split up into a band of coloured rays, with the red ray at one end and the violet ray at the other. But in the course of his experiments Wollaston made a discovery that may truly be said to be one of the most important of the present century. He found that the band of coloured rays was divided, here and there, by dark lines.

Valuable as this discovery was, its importance was not fully recognised until another experimenter had worked at the same subject for some time. This was the now celebrated optician Fraunhofer, who more or less completely mapped out these dark lines and showed their number. By viewing the spectrum through a telescope Fraunhofer found these dark lines invariably to occupy certain definite positions when the sun was at, or near, its meridian; and as a consequence he was able to number and classify them. Since Fraunhofer's time the subject has made rapid strides, and although the labours of many scientific workers have contributed to this advance it is principally to the investigations of two German Professors (Professors Kirchhoff and Bunsen) that we are indebted for the extensive and intimate knowledge of this subject we now possess.

The meaning and cause of the dark lines in the solar spectrum were, at first, very imperfectly understood, and it is to Professor Kirchhoff that the credit of being the first to discover the cause of their being present is due. Kirchhoff found, in experimenting with the salts of the various metals, that under the same conditions the incandescent vapour of the salt of any given metal gave rise to its own form of spectrum, but to no other. He also found that the bright lines thus artificially produced, in any given case, were identical in position with some of the dark lines of the solar spectrum, and that with the salt of the same metal these lines were always in the same position. When a flame, coloured by a salt of sodium, was placed before the slit in the spectroscope, the yellow line to which this metal gives rise was found to occupy the exact position of the double line D, or what is now known as the sodium line in the solar spectrum. Following out his experiments still further, Kirchhoff found that if the light, emanating from the burning of any given metal, was passed through the vapour of the same metal the coloured line on its spectrum became a dark line; or, in other words, the colour became annihilated. In explanation of this, it is now said that if a coloured ray of any given degree of refrangibility is allowed to pass through the vapour of any metal having, in the light it gives off, the same degree of refrangibility, the colour becomes absorbed. In passing I may remark that this is hardly an explanation, but simply one way of stating the fact. It is undoubtedly an example of the power that one body has of destroying a phenomenon that another body with similar qualities has set up.

Mr. Norman Lockyer is also a very ardent worker in this department of research, and it is largely owing to his experiments that we get a correct idea of the way in which the atmosphere surrounding the sun produces the dark lines in the solar spectrum. According to Mr. Lockyer, it would seem "that around the sun, though at an immense distance from it, there is a cooler atmosphere from whence comes that circle of light, or corona, as it is called, which radiates into space in all directions round the hidden sphere at the time of total solar eclipse. Immediately below this cooler atmosphere, and consequently nearer the sun, there is now known to be a chromosphere, as it is termed, of incandescent hydrogen, with intensely glowing vapours of calcium and magnesium of such enormous volume and such stupendous turmoil, that red bursts of flaming hydrogen have been seen to rise within a few minutes interval to a height of twenty-seven thousand miles!

"Below this chromosphere, and yet nearer the sun, are vast quantities of metallic vapours, such as sodium, iron, zinc, copper and other elements. . . . . This is what Mr. Lockyer designates the reversing layer, from its transforming into dark lines, by absorption, portions of the sunlight shot through it. . . . . Underneath the reversing layer is the intensely heated liquid or solid matter which is known as the photosphere, and which gives off the light thus fortunately robbed of its luminous qualities in passing outwards."* It is fortunate, because by the absorption of the light given off in the manner described it became possible to determine the sun's physical constitution as well as those of some of the planets and fixed stars. But not only the constitution of these bodies has been shown but their movement also.

Dr. Huggins, who is another of the enthusiastic workers in this department of research, has demonstrated to us the enormous speed at which some of the heavenly bodies move through space. By the use of the spectroscope he has shown that the dog-star, Sirius, which at one time was supposed to be a fixed star, is really receding from us at the rate of a *thousand millions of miles* in each year. This star, although one of the nearest of the fixed stars, is so distant from the earth that the light that comes from it is twenty years in reaching us; and although at this enormous distance from the earth, its movement and the rate at which it travels have been ascertained by the use of this wonderful modern instrument the spectroscope.

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* *The Modern Seven Wonders of the World.* By Chas. Kent. London: Geo. Routledge & Sons. 1890. Chap. v.

Thus it has been seen that the discovery of Fraunhofer's lines, and of the mode in which these lines are produced, led to the discovery of the mode of ascertaining the physical constitution of the sun and other heavenly bodies, whilst at the same time it led to the possibility of demonstrating the movement of some of these bodies through space at a speed that seems to bid defiance to any attempt to form a clear idea of the rate of progression or of the immensity of the regions through which they move.

In the earlier experiments that led the way to these discoveries, the attention of the scientific world was principally devoted to ascertaining and verifying the leading facts of spectroscopic analysis, without reference to the quantity of material used in making the demonstrations. But after a time attention began to be given to this phase of the question, and by degrees it was found that the quantity of incandescent metal that was being used in any given case, could be largely diminished without in any way affecting the result, so far as the production of its own colored line on the spectrum was concerned. In the course of time it was proved that *one five-millionth* of a grain of sodium in a state of incandescence would with unerring certainty produce its specific yellow line on the spectrum. But this quantity, minute as it is, is much larger than is needed. It is now stated that *one two hundred millionth of a grain*[*] will give the characteristic line of this metal with as much certainty as the larger quantity.

Thus we have seen that upon the existence of the so-called reversing layer in the sun's atmosphere the possibility of proving the presence of certain metals in the sun and its surroundings entirely depends, and we now see that the demonstration of the truth of this proposition can be made with a quantity of the active agent that everyone must admit to be infinitesimal.

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[*] Vide *Insectiverous Plants*, by Chas. Darwin, M.A., F.R.S. Footnote page 170. (Quoted by Darwin from Balfour Stewart *On Heat.*)

We pass on now to a department of research in which the chemical action of light plays a most important part, namely, *photography*. Everyone in our day is more or less familiar with the way in which the camera will depict on the glass screen beautiful though fleeting pictures of any objects that come within its range, and it is perfectly natural that there should have arisen in the minds of scientific men a strong desire for the invention of some methed of fixing and rendering permanent these pictures in their natural colours. Some advances have been made in this direction,* but it is to be feared that the perfect solution of this problem is still a question for the future. Great advances have, however, been made in the mode of producing monochromatic pictures by the aid of photography.

For some three hundred years it has been known that the salts of silver darkened under the influence of light, but it is only in comparatively recent times that attempts were made to utilise this property of silver salts. Thos. Wedgwood (the son of Joshua Wedgwood, of Etruria) along with his friend Sir Humphrey Davy, were apparently the first observers that attempted to make any use of these salts for the purposes of photography. These attempts were, however, successful only to a limited degree.

The process invented by Daguérre was a great advance upon anything that had been done before, but even this was comparatively useless for many purposes on account of the length of exposure the plates needed in order to secure a perfectly developed image. The perfection of the process and the shortening of the time of exposure necessary were entirely due to the accidental discovery of the action of mercury on the prepared plates. As is well known Daguérre's plates were silver (or silvered) plates that had been subjected to the action of the vapour of iodine. This made them more sensitive to the action of light, and when put into the camera an image was developed upon them after a time, but, as I have said above, the length of time necessary made the process useless for some purposes. An account of the accidental way in which the properties of mercury were discovered is not without interest to us, and is, I think, worth giving.

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* It is confidently stated that Herr Franz Veress's process of photographing in colours is based on that suggested by Mr. Carey Lea:—namely, the application of chlorides of silver in the form of an emulsion. The photographs are taken on glass and paper, and the tints range from a deep red, through yellow, to bright blue, but green is absent in all the positives. The sensitive preparation is a silver chloride emulsion in collodion or in gelatine, and the solution, which is kept a secret, is poured on the plate, where it takes a reddish colour. A long exposure is required, it is said two or three hours for a picture taken in a copying frame, and *over a week* for a *landscape* taken in the *camera*. The process will be of little use unless the exposure can be reduced from weeks to minutes.—*Photography*, July 17th, 1890.

Daguérre had placed aside in a closet some plates that had been exposed too short a time, and that had no image upon them, and on this account were considered to be useless. Looking at the plates some time after, Daguérre found an image upon them, and supposed this was due to the action of some chemical substance stowed away in the closet. He had the chemicals removed and placed other half exposed plates in the closet. Images were again after a time produced, and he was greatly puzzled to account for the phenomenon until he discovered a dish of mercury, on the floor of the closet, that had been overlooked. As mercury gives off vapours even at ordinary temperatures, he supposed this might be the cause of the change.

"To test the accuracy of this supposition he again took a plate that had been exposed a short time in the camera and on which no image was visible yet. He exposed this plate to the vapour of mercury, and, to his intense delight, an image appeared, and the world was in this way enriched by a most beautiful discovery."* I think, too, the world was also indebted to the action of a very minute, if not an absolutely infinitesimal, quantity of material for the perfecting of the process.

But the principal thing which I wish to call your attention to in connection with this subject is the alteration that has taken place in the length of time necessary to expose a plate in the camera in order to produce a perfect negative. In the early days of the art it was difficult to ensure perfect stillness in living objects long enough to enable a perfect image to be obtained. This led to a continuous effort to shorten the time of exposure, by increasing the sensitiveness of the material on the plate on which the negative is taken; thus lessening the quantity or dose of light needed to produce a given effect. So great has been the success in this direction, and, if I may use the term, so infinitesimal is the quantity of light now needed to produce a perfect negative, that it is very difficult to form a correct idea of the extremely minute portion of time it takes.

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* *The Chemistry of Light and Photography.* By Dr. Herman Vogel. London : Kegan Paul, Trench & Co. 1888.

The small instrument I hold in my hand is one of Kershaw's instantaneous shutters, and is arranged to allow the light to play upon the plate in the camera for about the one-hundredth part of a second. This instrument when put in motion will give you some notion of the infinitesimal quantity of light that is sufficient to do its work on the negative. But rapid as the action is here shown to be, it is far exceeded in some cases. It is now said that an exposure of *one seven-hundredth part of a second** will, under proper conditions of the light, produce a perfect negative. This, I think, we may also claim as the effect of an infinitesimal quantity of the active agent.

We pass on now to notice an invention which is perhaps one of the greatest wonders of the present century; I allude now to the phonograph, or the graphophone, as it has been indifferently termed. Like many other inventions, it seems to have been gradually developed, and in its most primitive condition gave little or no indication of the marvellous perfection it would arrive at. Its earliest form was probably the telephone, and it is now difficult to say where the one ended and the other began. The first telephone is believed to have been invented by Robert Hooke, more than two hundred years ago. This consisted simply of a couple of circular lids, each of which had a membrane of some kind stretched over it, the two being connected by a wire. Sir Chas. Wheatstone, in 1819-20, invented a similar instrument, but instead of a wire being made to connect the two diaphragms he used a deal rod. This invention was employed to convey the sounds of musical instruments from one room, in which the instruments were placed, to another in which the audience listened, and is said to have created a great sensation at the time.

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* *The Chemistry of Light and Photography.* By Dr. Herman Vogel. London: Kegan Paul, Trench & Co. 1888. p. 293.

From one point to another in the hands of different inventors, the development of this instrument has gone on to the point at which it has now arrived. Next to the phonograph itself, one of its most wonderful outcomes is the microphone. "The merest accident—the snapping of a fine wire during the course of an experiment—led to the discovery, by Professor Hughes, of that marvellous magnifier of minute sounds. . . . A watch, placed in a proper position, will yield extraordinarily loud sounds. . . . . As remarkable an experiment as any that can be made, however, . . . is to imprison a fly, a gnat, a moth, or any other small insect in a common match box, over a hole previously cut, in one side of which a piece of muslin, or a sheet of straw paper has been stretched. Upon placing the box, so arranged upon the stand of the microphone, you will not only at once hear the previously inaudible tread of the fly, but you will hear it tramping about as though it were a horse or a bullock. . . . . It does by the sense of hearing, in fact, what the microscope does by the sense of sight, it magnifies what without its help would be absolutely indistinguishable."* Another form of this instrument is the sphygmophone, by the aid of which the sound of a patient's pulse can be heard all over the room in which he is sitting.

I need hardly point out that, in the matter of sound, we here descend into the region of the infinitesimal, and yet by the help of this instrument these sounds are made perfectly audible. But wonderful as are the results obtained by the invention of the microphone, they are, in another way, completely eclipsed by the achievements of the phonograph. Different individuals have, at various times, put in a claim for the inventions and improvements that have helped to make the phonograph what it now is, and no doubt some of these claims are just and proper; but if there is any one individual that has contributed in a greater degree than any other to the improvements that have helped to make the phonograph what it is, that individual is the now celebrated Thomas Alva Edison, of New York. In one of its earliest forms his phonograph consisted of a cylinder of iron with a spiral groove cut upon it. Around this a sheet of paper was wrapped. By the aid of a screw a diaphragm with a small stylus or point attached to it was made to traverse from one end of the cylinder to the other. If the diaphragm was made to vibrate by the sound of the voice or any musical instrument, the vibration was conveyed to the style, and indentations were made in the paper around the cylinder. These, when once made, would reproduce the sounds if a point attached to the diaphragm was carried over them again by the cylinder being made to revolve. Edison after a time used tinfoil in place of the paper, but this, like the latter, was found to alter so rapidly in repeating the speeches recorded upon it, that it was soon superseded in favour of a wax cylinder. This was of a texture sufficiently yielding to permit a point to cut its way, and thus make indentations on its surface, and yet sufficiently hard to allow another point to go over it again an indefinite number of times, if the sounds recorded upon it had to be reproduced. So delicate is the adjustment of the point, and so infinitesimal is the wear and tear in reproducing the sounds, that it is said that Mr. Edison has cylinders by him that have, in each case, reproduced the speeches recorded upon them several thousand times without showing any very decided signs of being worn out.

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* *The Modern Seven Wonders of the World.* By Chas. Kent. London, Geo. Routledge & Sons. 1890.

Through the kind help of a friend I have obtained the loan of a cylinder that has been used in one of Edison's machines, and with your permission I will pass this round for your inspection whilst I make one or two observations on the depth and quality of the indentations we see upon it.

In the first place you will, if your eyesight is good, notice that a plain smooth line passes round the cylinder in a spiral direction before any indentations commence. This smooth line is caused by the point or style pressing very gently upon the cylinder before any sound from the voice is heard; but the moment a word is uttered loud enough to be heard, the diaphragm or disc, to which the style is attached, is set in vibration and the word is faithfully sculptured upon the cylinder in the way that you see. And at this point I wish especially to call your attention to the smallness of those indentations that are visible to the naked eye. Beyond these latter, however, there are some that can only be discerned by the aid of a glass, and in some of them the amplitude of the vibration is extremely small—in fact almost infinitesimal. But however minute these are, they are, with the aid of a set of smaller or subsidiary waves, (to which I shall refer presently) capable of reproducing the sounds that caused them with an exactness that is almost startling the first time you hear them. And those that have for the first time heard the voice reproduced in this way can well understand the sudden exclamation of bewildered surprise that came from one of Edison's workmen the first time he heard the phonograph utter articulate sounds. The account of this I must give you in Edison's own words. After having conceived the idea that a machine could be made that could not only record the sounds in the way indicated above, but that would also reproduce them, he determined to try to accomplish this. Having carefully made his preparations, Edison put the thing to the test, and to his intense satisfaction found the sounds were reproduced with a marvellous distinctness and correctness. "Looking about, Edison, seeing one of his foreign workmen standing not far off, called him to him," and after having put the point to its original position on the cylinder he directed the workman to bend his head down and listen after the machine had been set in motion. Edison, in describing the effect, says: "I watched that Dutchman's face, and it was a curious thing to observe the changes of expression that came over him. Finally he straightened himself, raised his hands, and said, 'Mein Gott, she speaks!' "*

I have just previously called your attention to the smallness of the amplitude of the vibrations in many of the indentations,† and to the fact of there being another or subsidiary set of vibrations that cannot be seen on the cylinder even by the aid of a glass. All sounds produce atmospheric waves of a simple or compound character, and these can be traced* by a method that is in some respects not unlike that by which we obtain tracings of the pulse wave. In those of a compound character there is a principal wave and also a number of subsidiary waves. This will be understood by a reference to the tracing of a pulse wave as produced by the compact and beautiful instrument — the sphygmograph — invented by our esteemed friend and colleague, Dr. Dudgeon. You are aware that in the pulse wave produced by this instrument, there are in the descending portion of the wave one or more smaller waves. In like manner there are small or subsidiary waves produced by sounds. In these, however, the smaller waves are often numerous and are more or less equally distributed over the principal wave. These subsidiary waves, according to their number, shape and arrangement, go to form the overtones, and (combined with the principal wave) produce what is known in musical language as *quality* or *timbre.* But beyond these, other variations, of a very infinitesimal character, occur in the form and arrangement of these smaller waves. If a voice sings the same note—say the note G—and repeats this note at the same pitch, using one of the vowel sounds each time until all the vowels have been used, the arrangement and form of the waves will be different in each case, notwithstanding that the singer is the same person and the note is the same pitch in all the trials.

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* *The Modern Seven Wonders of the World.* By Chas. Kent. London: Geo. Routledge & Sons. 1890.

† I speak here only of the amplitude of the vibration of the lever that produces the indentation, not of the amplitude of the atmospheric wave.

Inasmuch as all these combinations and peculiarities of tone can be reproduced by the phonograph it follows that they must have been recorded on such cylinders as the one I have shown to you, and it also follows that the record must have been made by exceedingly infinitesimal variations in the form and number of the waves. But I must ask you to go a step further with me :—

Vibrations below or above a certain number per second give no sound to the human ear. The upper limit is said to be about thirty-eight thousand per second.†

If, then, we assume that one tenth of this number (or say four thousand) of principal waves can be recorded upon a cylinder such as I have shown you, and then consider that in some cases six times the number of the smaller waves will, of necessity, have to be recorded at the same time, it requires no stretch of the imagination to come to the conclusion that we are here dealing with infinitesimal quantities, and that these are constantly doing their work in the world.

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* By the use of Scott's Phonautograph.

† *The Theory of Sound in Relation to Music.* By Professor Pietro Blaserna. London : Kegan Paul, Trench & Co. 1883.

We come now to a department of nature in which in its earliest, if not in all its stages of being, the infinitesimal reigns supreme. The organisms included in this department are designated by the term *microbe*, a term that was invented in order to get rid of the difficulty of having to determine off-hand whether any given organism belonged to the animal or to the vegetable kingdom. Whether we regard it from the physiological or chemical point of view, in no department of scientific research have more important changes occurred than have been seen in this.

During the latter part of the middle ages, even amongst learned men, it was believed that animals of a comparatively high order could be created *de novo* without the intervention of a living germ ; and even as late as the seventeenth century some authors described the method of producing frogs from the mud of marshes or of eels from the waters of our rivers.* But some of these errors were soon corrected by the spirit of scientific enquiry that invaded Europe in the sixteenth and seventeenth centuries. Some errors, however, remained, and when the microscope began to take its place as an important agent in scientific investigation, the doctrine of spontaneous generation was again revived. The microscopes of that day were only able to detect the fully developed forms of some of the larger microbes, but had not the power to penetrate beyond and reveal the earliest form of the germs of these creatures. Thus it came to be believed that they were generated spontaneously from non-living matter. About the year 1745 this question began to be keenly debated by the Italian physiologist —the Abbé Spallanzani—as the opponent of the doctrine, on the one side, and by Needham, a very able English observer, on the other. Needham endeavoured to prove by repeated experiments that living organisms could be generated after all the germs had been destroyed by heat. On the other hand, Spallanzani tried to prove by equally careful experiments that no living organism could be produced under any circumstances unless the germ of this organism was previously present. We had thus the curious spectacle of two observers, of excellent repute in the scientific world, placing their faith upon apparently well conducted experiments, and still arriving at opposite conclusions. But, as I shall presently show, this was largely due to the extremely infinitesimal condition of some of the earliest forms of these germs.

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* Leuwenhœch. *Epistola.* 1692.

Later on the controversy was renewed by M. Pouchet, a naturalist of Rouen, as the advocate of spontaneous generation; and by the now celebrated M. Pasteur, of Paris, as the opponent of this doctrine. In the present day the doctrine finds very few advocates, but is perhaps not quite extinct. No living observer has, however, done more to convince the scientific world of the unsoundness of the doctrine than the last-named observer. It would take me too far away from the main object of my paper to pursue this subject or to name even a few of the contributions by which Pasteur has made his name famous throughout the scientific world in investigating the action of microbes on living organisms. I must, however, allude to one matter that has some bearing on the question we are considering.

Liebeg, in Germany, had revived the doctrine that "the ferments are all nitrogenous substances—albumen, fibrine, caseine—or the liquids that embrace them—milk, blood, urine—in a state of alteration which they undergo in contact with the air."* The oxygen of the air was, according to this system, the first cause of the molecular breaking up of nitrogenous substances. The molecular motions were supposed to be "gradually communicated from particle to particle in the interior of the fermentable matter, which is thus resolved into new products." This theory held sway for many years. "Books, memoirs, dogmatic teaching, all were favourable to the theoretic ideas of Liebeg."* Pasteur's investigations were, however, destined to alter all this, and to show that fermentation was in many cases due to the presence of an extremely minute living organism. Pasteur, in making known these infinitely small organisms as the cause of one of the modes of fermentation, "had discovered a third kingdom—the kingdom to which these organisms belong which, with all the prerogatives of animal life, do not require air for their existence, and that find the heat that is necessary for them in the chemical decompositions they set up around them." *

Liebeg was a persistent opponent of this doctrine to the last, and refused to be convinced by the simple and yet beautiful scientific experiments Pasteur carried on from time to time.

The extreme minuteness of the spores of many of these microbes furnished, as I have said above, one of the greatest difficulties with which the investigation of their action was surrounded. More than one of the advocates of spontaneous generation had demanded that if these organisms infested the atmosphere to the extent they were supposed to do they ought to be able to be collected and weighed. Few even of the scientific men of that day seemed to have any exact notion of the weight of these germs, and even now, when so much has been done in investigating the life history of some of the microbes, it is almost impossible to form a correct idea of the weight of the germ in its earliest state.

I have myself succeeded in weighing some of the smaller organisms.† The pollen grains of the grasses vary in weight from one four-millionth to one ten-millionth of a grain. Some of those belonging to other natural orders vary from one thirty-millionth to one forty-millionth of a grain. But the spores of some of the cryptogams are exceedingly minute. In one of the fungi that resembles the edible mushroom, the spores weighed rather less than *one five hundred millionth of a grain.* These are probably nearly twenty times as heavy as the spores of the *Penicillium glaucum*, which I have never yet succeeded in weighing accurately. But these latter are perfect leviathans compared with the spores of the microbes.

And here at this point I must call your attention to the series of elaborate and extremely interesting researches on the life history of the monads, made by my esteemed friend Dr. Drysdale, in conjunction with his friend and co-worker, Dr. Dallinger.* In these researches animal matter was macerated in water until monads had been generated and had grown to the fully developed form. One of these was kept under continuous observation until it had gone through all its changes and had discharged its brood of young monads or spores. So minute were these that in speaking of them Dr. Dallinger and Dr. Drysdale say: "It became now a matter of great interest to study the future of these infinitesimal spores. With $\frac{1}{25}$ (of an inch objective) the most accurate observer could not have discovered their presence if he had not previously seen them with the $\frac{1}{50}$." This latter, however, with an A eyepiece gave a magnifying power of *two thousand five hundred diameters*, and yet with this enormous power these spores appeared as a mere nebula under the object glass.

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* *Louis Pasteur, his Life and Labours.* By his Son-in-law. Translated from the French by Lady Claude Hamilton. London : Longmans, Green & Co. 1885.

† For my method of doing this I must refer the reader to the last edition of my work on Hay-fever.

It is exceedingly difficult to give even an approximative idea of the size of such spores, and quite impossible to give anything like a correct estimate of their weight, but from what I have said above, of the weight of other spores that are immensely larger, it will be seen that those of the monads referred to, must be infinitesimal in the highest degree. It is, moreover, certain that, if some hundreds of millions of these organisms were present in the blood stream, and could be separated at will, this number would not affect the best balance that has ever been constructed.

It is now admitted on all hands that microbes set up disease when they obtain an entrance into the blood stream, but it has not yet been determined, in all cases, whether the spore, in its earliest form, or the fully developed microbe has the greatest share in producing the disturbance that they cause. Nor yet is it fully determined whether the product of the generation of these organisms in the form of a *ptomaine* is not in some cases a principal cause of disturbance. It is, however, tolerably certain that the number I have named above could not be present in the blood without setting up a considerable amount of disturbance, although the actual quantity, as far as weight is concerned, would be infinitesimal in a high degree.

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* *Researches on the Life History of the Monads.* By the Rev. W. H. Dallinger, F.R.M.S., and J. Drysdale, M.D., F.R.M.S. Reprints from the *Monthly Microscopical Journal.*

Although the opinion has been contested by some observers it has been stated by Laveran and Richard—two military surgeons—that a microbe is found in the blood of patients affected with intermittent fever. Richard says that the multiplication of these bodies must be extremely rapid, as " they are not found in the intervals of the attacks (apyrexia). As the attack approaches they appear in increasing numbers, and their maximum corresponds with the beginning of the rise in temperature; from that moment they begin to perish, since the heat of fever is fatal to them and completely checks their development. They produce fever, the fever kills them, and then subsides; when apyrexia occurs they multiply again, excite fever, and so on."*

The ptomaines, in their virulence and in their mode of action, are said to resemble the snake poisons, and we all know how rapid and deadly some of these latter are even in very minute quantities, and it is therefore reasonable to suppose that an infinitesimal quantity may set up serious disturbance.†

There is also another way in which the generation of morbid symptoms may be aided or hindered, namely, by the action of the leucocytes of the blood. " Steinberg was the first to suggest that it appears to be the function of the colourless corpuscles to take possession of the bacteria introduced into the blood and to destroy them. We know in fact that the colourless corpuscles do take possession of all foreign particles, such as micrococci and bacteria, introduced into the blood, and in a sense encyst them in their protoplasm. When the bacteria multiply in the blood they must necessarily have an irritating effect on the walls of the blood capillaries, and this appears in the swelling of the cells and their return to the spherical form; in a word, they are transformed into embryonic or migratory cells (according to Cohnheim's theory)."‡

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* *Microbes, Ferments and Moulds.* By E. L. Trouessart. London: Kegan Paul, Trench & Co. 1886. P. 185-6.

† The Monograph of our esteemed friend Dr. Hayward shows this in a very forcible and interesting manner. *Vide* article "Crotalus" in *Materia Medica, Physiological and Applied.* By Jno. W. Hayward, M.D. Vol. I.

‡ *Microbes, Ferments and Moulds.* By E. L. Trouessart. London: Kegan Paul, Trench & Co. 1886. P. 235.

This phase of the subject has had still further development given to it by the researches recently made by Metschnikoff, a Russian physiologist. Whilst working in Pasteur's laboratory in Paris he has discovered the reason why the millions of microbes that float in the atmosphere do not in larger numbers penetrate into the blood vessels and light up disease. According to an article in *The Speaker*, written by Sir Henry Roscoe, "Metschnikoff has proved that certain cells contained in the blood of the higher animals, termed phagocytes, identical with the well-known white blood-corpuscles, being endowed with the power of independent motion, not only wander inside but even make their way outside the tissue, and, *mirabile dictu*, pursue, devour, and digest any bacilli, whether poisonous or not, with which they come in contact. This, then, is the new and true battle of life which, hitherto unknown and unobserved, is constantly going on within the body. . . . This is no fancy picture. It is a struggle for existence which can be followed in all its phases by anyone who takes the trouble to look for it. . . . This apparently independent life of the cell within the organism is one of the most marvellous revelations of modern science."* This sentiment I am sure, gentlemen, you will endorse, as I do myself, in the most emphatic manner.

I might here enlarge on the influence that such discoveries as these have in showing the wisdom of our mode of attenuating and administering our medicines, but this is too wide a subject to be entered upon on the present occasion. It is not known what the exact proportion of the leucocytes is to the whole volume of blood, but it is estimated to be exceedingly small. And if the *phagocytes* are only a small proportion of the whole number of white blood-cells I think we shall, for the reasons I have given above, be justified in claiming this discovery of Metschnikoff's as an excellent example of an infinitesimal quantity acting upon an infinitesimal quantity and thereby preventing disease.

In a paper read by me some years ago at one of our Congresses, amongst other matters I alluded to the experiments of the late Mr. Darwin on Insectivorous plants. At that time we had very little of the details of the experiments given; but since the publication of his life and letters by his son* we have all the particulars by which he arrived at his conclusions given. I propose briefly to notice these and to call your attention to the results of his careful investigations.

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* *The Speaker*, August 16th, 1890, p. 179-80.

In his autobiography, Darwin says: "In the summer of 1860 I was idling and resting near Hartfield, where two species of *Drosera* abound; and I noticed that numerous insects had been entrapped by the leaves. I carried home some plants, and, on giving them insects, saw the movements of the tentacles, and this made me think it probable that the insects were caught for some special purpose. Fortunately a crucial test occurred to me, that of placing a large number of leaves in various nitrogenous and non-nitrogenous fluids of equal density, and as soon as I found that the former alone excited energetic movements, it was obvious that here was a fine new field for investigation."†

Darwin followed up these investigations and gradually brought out results that greatly surprised and apparently troubled him. In writing to his friend Dr. Gray about some of his earlier experiments, Darwin says:—"I have been infinitely amused by working at the *Drosera;* the movements are really curious, and the manner in which the leaves detect certain nitrogenous compounds is marvellous. You will laugh; but it is, at present, my full belief (after endless experiments) that they detect (and move in consequence of) the $\frac{1}{2880}$ part of a grain of *nitrate of ammonia.*"‡

Later on, in writing to another friend, he says: "I had measured the quantity of weak solution and I counted the glands which absorbed the *ammonia* and were plainly affected; the result convinced me that each gland could not have absorbed more than $\frac{1}{64000}$ or $\frac{1}{65000}$ of a grain. I have tried numbers of other experiments all pointing to the same result. Some experiments lead me to believe that very sensitive leaves are acted upon by much smaller doses."‡

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* *Life and Letters of Charles Darwin, M.A., F.R.S.* Edited by his son, Francis Darwin. London: Jno. Murray. 1887.

† *Life and Letters of Charles Darwin, M.A., F.R.S.* Edited by his son, Francis Darwin. London: Jno. Murray. 1887. Vol. I., p. 95.

‡ *Life and Letters.* Vol, III., p. 318-19

Again, in writing a little later on in the same year to his friend Sir J. D. Hooker, he says: "I have been working like a madman at *Drosera.* Here is a fact for you, which is as certain as you stand where you are, though you won't believe it, that a bit of hair $\frac{1}{78000}$ of a grain in weight, placed on a gland, will cause *one* of the gland bearing hairs of *Drosera* to curve inwards, and will alter the condition of every cell in the foot-stalk of the gland."*

A few days later, writing to his friend Lyell, he says: "I will and must finish my *Drosera* MS., which will take me a week, for at present I care more about the *Drosera* than the origin of all the species in the world. I will not publish on *Drosera* till next year, for I am frightened and astounded at my results. I declare that it is a certain fact that one organ is so sensitive to touch that a weight seventy-eight times less than that—viz., $\frac{1}{1000}$ of a grain—which will move the best chemical balance suffices to cause a conspicuous movement."*

Thus Darwin went on from step to step carefully trying the effect of smaller and smaller quantities of *ammonia salts.* In a letter to Dr. Burdon Sanderson, he says: "I must tell you my final result of which I am sure [as to] the sensitiveness of the *Drosera.* I made a solution of one part of *phosphate of ammonia* by weight to 218,750 of water; of this solution I gave so much that a leaf got only $\frac{1}{15520000}$ of a grain; this being absorbed by the glands sufficed to cause the tentacles bearing the glands to bend through an angle of 180°."* Again, in writing to his friend Asa Gray about the smallness of the dose of *phosphate of ammonia* that would move the tentacles he says: "No human being will believe what I shall publish about the smallness of the doses of *phosphate of ammonia* which act."†

Having been told by his son that Professor Donders had stated to him that so small a dose as the one-millionth of a grain of *atropine* would act upon the eye perceptibly, Darwin wrote to Professor Donders as follows:—"Now will you be so kind, whenever you can find a little leisure, to tell me whether you yourself have observed this fact or believe it on good authority. . . .

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* *Life and Letters.* Vol. III., p. 319-20-24.

† *Life and Letters.* Vol. III., p. 325.

The reason why I am so anxious on this head is that it gives some support to certain facts repeatedly observed by me with respect to the action of *ammonia* on *drosera.* The $\frac{1}{4000000}$ of a grain absorbed by a gland clearly makes the tentacle which bears the gland become inflected; and I am fully convinced that $\frac{1}{20000000}$ of a grain of the crystallised salt (*i.e.*, containing about one-third of its weight of water of crystallisation) does the same. Now I am quite unhappy at the thought of having to publish such a statement. It will be of great value to me to be able to give any analogous facts in support."*

Professor Donders subsequently corroborated his statement, and Darwin fully confirmed the conclusions he had arrived at. But he even went beyond his estimate of $\frac{1}{20000000}$ of a grain, and calculated that if we deduct the amount of the water of crystallisation from the dose of the salt administered to each gland the quantity of active material would be less than $\frac{1}{30000000}$ of a grain.

In his concluding remarks on this part of the subject in his work *On Insectivorous Plants* Darwin says:—"There is nothing remarkable in the fact of one-twenty millionth part of a grain of the phosphate, dissolved in about two million times its weight of water, being absorbed by a gland. All physiologists admit that the roots of plants absorb salts of *ammonia* brought to them by the rain; and fourteen gallons of rain water contain a grain of ammonia, therefore only a little more than twice as much as the weakest solution employed by me." He then goes on to say that the wonderful fact is that $\frac{1}{20000000}$ part of a grain (including less than one-thirty millionth of efficient matter) should cause the basal part of a gland to bend through an angle of above 180°.†

At the risk of being somewhat tedious I have given many of the details of the experiments tried by Darwin. Evidently the results both surprised and troubled him. In the hands of a man of smaller scientific reputation they certainly would not have been all at once accepted, but with an observer of such celebrity, and with all the care that he evidently took to make all his conclusions certain, there could be no ground for having the slightest doubt about his results.

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* *Life and Letters.* Vol. III., p. 325.

† *Insectivorous Plants.* By Charles Darwin, M.A., F.R.S., pp. 172-73.

Though the *Drosera* is apparently not endowed with a true nervous system, the action of the *ammonia* seems to have been as normal as if a nerve tissue had been present in the gland. Darwin came to the conclusion that a continuous line of protoplasm served the same purpose and transmitted motor power to the base of the foot-stalk of the gland. However this may be, the experiments furnish an excellent example of the action of infinitesimal quantities even on an organism that is low down in the scale of creation.*

With one exception the facts I have so far brought under your notice lie somewhat beyond the pale of medicine proper. I must now, in conclusion, notice one example of the power that very minute quantities of a morbific agent that does not belong to the zymotic class have in bringing on disease; and in doing this I must be permitted to refer to some of my own investigations.

In the second edition of my work on hay-fever I show that the $\frac{1}{40000}$ of a grain of pollen inhaled in each twenty-four hours will commence the malady, and that $\frac{1}{3427}$ of a grain will suffice to keep it up at its highest point of intensity: but one of my reviewers in 1880 expressed great doubt about the accuracy of my conclusions, and intimated that these quantities could not represent the quantity inhaled under some circumstances. The experiments had been very carefully made, and as nearly as possible represented the average quantity of pollen a patient would inhale in pursuing his daily avocations, unless his duties compelled him to be in the midst of hay-grass during the whole period of its flowering.

A case that came under my notice a little time ago caused me to determine to ascertain what was the largest quantity of pollen a patient would inhale if kept in the midst of flowering grass during the whole of the hay season, and for reasons that I shall presently give this case was much more interesting than an ordinary one. The patient, a lady, residing in the west of England, was a terrible sufferer from hay asthma, and her husband came over* to consult me on her account. The previous season (1886) had been spent in the now celebrated island of Heligoland, where the patient was tolerably well during the whole time she remained, but from the description given to me I should imagine this was one of the worst cases of hay asthma I had ever been consulted upon. Her home was in the very midst of land used for the growth of hay grass, so that here in the hay season we should have a large quantity of pollen generated. The object of the consultation was not so much that of treatment by medicine, as to see if it would be possible to prevent the pollen gaining access to the apartment in which the patient was sitting, and thus to avoid the necessity of her leaving home at a given time each year whatever, in other respects, her state of health might be. I at once gave the opinion that this could easily be done if a proper apparatus was used, and gave a sketch of one that I thought suitable. This consisted of a square tube of wood, ten inches by ten, in which were placed three muslin screens (double layers) moistened with glycerine and carbolic acid. This the gentleman had fitted up, and at the same time had an air propeller fixed so that a current of air could be driven through the tube as often as desired.

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* *Insectivorous Plants.* By Charles Darwin, M.A., F.R.S. Pp. 172-3.

For two years the experiments were carried out at Ramsgate, and were fairly successful, but this year they have been carried out at their own home in Gloucestershire. An extra room has been added to their house, and a small gas-engine has been put down to drive the air propeller. A trial of the apparatus has been fairly made this year and appears to have been very successful. The husband of the patient writes me and says:—"I keep the engine going twelve to fourteen hours a day, thus keeping the room well supplied with fresh air. I think it due to you to inform you what the result has been, and I know you will be pleased to hear that though in the midst of grass and hay-making she (my wife) has had no symptom at all of hay-fever."

The question with me in this case was what was the largest quantity of pollen the patient could have inhaled each day if she had been left in her usual condition when at home during the hay season? As obviously I could not go to Gloucestershire to follow out experiments there, I did the next best thing by selecting a locality surrounded by land devoted to the growth of hay-grass, and in this respect closely resembling the one in which the patient resides. Here a fresh set of experiments were carried on. The apparatus I used was a very simple one and consisted of a flattened glass tube, with a row of microscopic cells top and bottom. Each cell was moistened with glycerine, and by inhaling through this tube every pollen grain was deposited in one or other of the cells; and the pollen usually was all deposited before the tenth pair of cells was reached—the largest number being of course detained in the first pair.

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*Sent, I believe, by my friend Dr. Shaw, of Hastings.

The experiments were commenced last year, when the grass began to be fairly in flower. One thousand inspirations through the tube were made at each experiment, occupying about an hour each, whilst at the same times the eyes and nostrils were protected. On two occasions breathing was carried on (after the inhalations through the tube) for the same length of time, but without the protection to the eyes and nostrils, in order to see what symptoms would be developed. The last occasion was when the grass was fully in flower, and the symptoms were so severe I was glad to conclude the experiment before the proper time was expired.

The minimum quantity obtained at the commencement of the experiments was $\frac{1}{240000}$ of a grain, and the maximum was $\frac{1}{30000}$ of a grain. So that the largest quantity the patient could have inhaled, in a day of ten hours, would have been one twenty-four thousandth of a grain as a minimum, and one three thousandth as a maximum. From careful and oft-repeated experiments, I am certain that so small a quantity as the $\frac{1}{100000}$ of a grain of pollen will give rise to very perceptible symptoms if this is inhaled within a given time. In fact this capacity for acting in so small a quantity lies partly in the fact that every pollen grain when it comes in contact with the mucous membrane is detached and has its own sphere of action, unimpeded by anything immediately in contact with it.

In the case I have given above we have a double testimony, viz., the severe suffering caused by the presence of an infinitesimal quantity of the exciting cause of the disease and the perfect freedom from this suffering when this infinitesimal quantity was taken out of the air the patient breathed. But this experiment has an additional interest to me. It has for many years been a favourite idea with me that it would be quite possible to free the air from the immense number of infinitesimal germs that float in it. Here we have a proof that this can be accomplished, and I cannot help thinking that this method of obtaining pure air will some day become a valuable help in the treatment of some diseases.

I have thus brought under your notice a few of the facts that have had a powerful influence upon my own mind in confirming my belief in the action of infinitesimal doses. These examples have been taken from various departments of scientific research, and give proof that in many of these departments the evidence of the action of infinitesimal quantities is to be found. In the cases I have cited, in the department of medicine proper I have shown that disease may be generated by the action of an infinitesimal quantity of the morbific agent; and in the last case this not of the zymotic class. It is, therefore, reasonable to believe that disease may be cured by the same class of agencies in very minute doses.

What may be the effect of a knowledge of facts like those I have cited upon the minds of our brethren of the opposite school I cannot say, but for myself I have no choice but to believe in the action of infinitesimal quantities, and I cannot but think that if our allopathic friends had the courage to look such facts as these in the face, and to act up to their convictions, we should soon have a great change in their opinion and practice. That this change will come (nay that it is rapidly coming) I am perfectly certain. Time and the march of investigation are fighting our battle, and the tendency of modern scientific research is to show that the action of the infinitesimal is a mighty agency in the world. Our obvious duty is to help on this investigation by every means in our power, and thus to hasten the day when the whole profession will be united in endeavouring to make the art of medicine a blessing to mankind greater than it has ever yet been.

Old Trafford,
Manchester.